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ETHER AND CHLOROFORM COMPARED AS ANÆSTHETICS.

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For a long time, the medical profession of Lyons, through its most competent representatives, has been approaching conclusions favorable to the employment of ether in preference to chloroform. Our readers will judge of the value of the reasons which were alleged in this judicious comparison. In our own opinion, ether deserves to be rescued from the neglect which befel it immediately after the first application of chloroform to general anæsthetic purposes.—[Note by the editor of the *Revue Médicale*.]

The Medical Society of Lyons has lately devoted two meetings to the examination of this important problem [the comparative value of ether and chloroform], which, in spite of the efforts of the medical profession, still remains in suspense over the lives of patients and the consciences of surgeons. In response to an opportune initiative of M. Barrier, all the members whose experience authorized them to do so, in turn rendered their testimony, which testimony, in each instance, ended with the same conclusion;—save in certain exceptional cases, absolute rejection of chloroform, and adoption of ether, for anæsthetic purposes.

The report of the discussion, which will shortly be published, will give the part taken by each speaker in this exposition of our surgical practice. We feel it to be a religious duty to promulgate these acts of the Medical Society. It was already known in the medical world that ether was preferred in Lyons, but it was perhaps doubted whether this preference was so general, we may say so unanimous, and it is necessary to inquire upon what considerations it is based. As much interest as profit will be excited by the significant history of conversions, the simple effect of praiseworthy scruples, wrought upon certain practitioners who for a brief period had the weakness to return to the use of chloroform. Especially should we reflect upon the explicit declarations of those of our colleagues who for nearly eight years have been able, by means of ether alone, to fulfil all the indications, however diverse, however numerous, of a hospital practice, which, for activity and

boldness, has no rival to fear. The example which the second city in France furnishes in this particular is of immense utility, and has the merit, equally incontestable, of opportuneness.

In fact, after having remained for nearly a year the only anæsthetic agent, ether was compelled to give place to chloroform; but six months had scarcely passed, before the case of M. Gorré, of Boulogne, gave the alarm to surgeons. I wish I could say it enlightened them, but I cannot; it is truly melancholy to see how, from that moment, this first warning of death was misunderstood and misrepresented.

In this case, where the patient, previously healthy, was a corpse after a few inhalations, M. Malgaigne positively refused to admit the slightest toxic effect of chloroform, and immediately every one present at the meeting of the Academy hastened to offer the support of his opinion, also in the negative. Some attributed the death of the patient to his alarm before the operation, others to the impurity of the chloroform which was used; M. Baillarger referred it to an intercurrent attack of syncopal epilepsy, Roux and Dupuy to a rupture of the lung and penetration of air into the veins. M. Velpeau even went so far as to inquire whether, in such a case, the danger did not arise from the means employed to dissipate the anæsthetic insensibility!

It ought perhaps to be admitted, in the defence of the Academy, that that body was not wholly free in the premises. Consulted officially by the Minister of Justice, it knew that its decision would weigh for or against the acquittal of an inculpated physician, and it was necessary to consider wisely reasons, which, uttered by certain orators too much interested in the object, might, so to speak, substitute scientific truth for fraternal veracity.

Since that time, however, for these ten years, the question is free from this embarrassment; it might be, and it ought to be elucidated. Too many fatalities, unforeseen, inevitable, irreparable, inexplicable, in the most skilful hands, have shown the uselessness of the different precautions upon which were built the hope of safety, which should be founded on a basis more solid than a calculation of probabilities. Nor has the practice been reformed, any more than the art was perfected. Every one administers chloroform according to the same rules which, exactly followed by his neighbor, cost him his patient; nevertheless he reassures himself, alleging for his sole motive that such a misfortune has not yet happened to *him*!

But a striking accident occurs, or two accidents happen at the same time; a sudden sensation is produced, an academic debate starts up, members deliberate learnedly on the question whether the death was the result of asphyxia, of sideration, of intoxication or of syncope; then follows a period of calm, until the occurrence of a new tribute which the *chloroformic scourge* will not fail, sooner or later, suddenly to claim.

Must I enter into details? Need I prove the essential importance of the score of prophylactics which have been, in turn, imagined? What can we hope from interrupted inhalations, when both pulse and respiration stopped in a patient of M. Nélaton (saved, nevertheless, by that skillful surgeon), who had hardly made a few inspirations, with five or six drops only of chloroform? What confidence can we place in apparatuses (almost unanimously rejected, by the way, by the Academy), when Dr. Snow had a patient die under his hands, with the inhaler; and when we remember, too, that in the great majority of cases chloroform is fatal in such minute doses that the safest inhaler must inevitably admit more?

It is true, that by an unexpected concession, marvellously concealed under the pretence of a triumph, the friends of chloroform are satisfied with inducing a less profound slumber, in order the better to ward off the danger. M. Faure has endeavored to establish, as a general method, the semi-anæsthesia, obtained by admitting air through one nostril, and the vapor of chloroform through the other—the mouth being kept closed. We should be thankful to praise this modification, if it really diminished the perils of anæsthesia in the same proportion as it curtails its benefits; but we cannot regard without fear the great embarrassment to the respiration caused by closing the mouth; and we hardly dare calculate the chances of asphyxia which the patient would run, if the nostril, which was to be the only open communication with the air, should be naturally or morbidly contracted. In another point of view, it seems singular to establish beforehand, for all subjects, and in every stage of insensibility, the dose of chloroform and that of ether, in equal proportions (excepting, for that of chloroform, a reduction corresponding to the thickness of the walls of the tube introduced into the nostril). This impossibility of increasing the dose of chloroform, to which the operator voluntarily condemns himself, has two inconveniences:—first, it retards the approach of sleep; and second, it exposes the patient, by the very slowness with which this is produced, to the danger of an accumulation of chloroform in the respiratory passages, which might prevent him from being roused so quickly as the supervention of alarming symptoms might sometimes require.

Yet, if each physician who had lost a patient would henceforth renounce an agent which might the next day cost him another, the personal experience, accumulating by degrees, would gradually preponderate. It might cost ten years more, and a hundred deaths, but neither the time nor this blood would be wholly lost! Unfortunately, this is not the case. In the moving throng in the midst of which they operate, our colleges, Parisians, English, Americans, find the opinion of the profession a support which is the more real, because each one knows that he may soon have need of such aid. The deaths therefore succeed each other, without any person being

converted by them; the grave closes, leaving hardly a trace, even in the memory of the surgeon. Yesterday he killed by chloroform, he will begin again to-morrow, with the same method, the same agent, the same indications!

In the midst of these contradictions, inexplicable to the humane and the sensible, an honest man has at last appeared; he insisted, and in the midst of the Société de Chirurgie, that chloroform should no longer be employed as an anæsthetic until the means of rendering its usage safe should be discovered. What reply was made to M. Hervez de Chégoin?

"There have been, within ten years, 68 cases of death from chloroform. But from these, we must eliminate 8, attributable to the inexperience of an early period."—(We know to what perfection the practice has since attained, and how much safety it can now promise.)

"We must also leave out 10 other cases, occurring either in town or in the country, in which we have no means of ascertaining that imprudence was not committed."

"The country," which, during the past four years, has read of no death from chloroform in France except in the cliniques of the hospitals of Paris, ought to thank its judges for only placing it, in this respect, in a position of suspicion.

These legitimate deductions being made, and the daily number of chloroformizations in Paris being approximately calculated, we obtain a proportion of 1 death out of about 6,000 patients chloroformed. Now, one out of six thousand, it is concluded, what is that, in comparison with the advantages realized by anæsthesia?

"I should have been strongly tempted to inquire, what then, under the point of view now in question, in respect to the mortality, are its advantages? Whether anæsthesia, which has caused the death of so many patients, has really, manifestly, by its exclusive influence, saved a single one? Whether the official statistics of amputations, for example, have notably changed within ten years? Whether public opinion, science itself, sees in anæsthesia anything else than a means of overcoming muscular contraction, and of mitigating the painful, but not mortal, sensations which accompany every surgical operation? Whether, far from adding to the safety of the patient, it ought not rather, as M. Ricord has said, to be considered as *a circumstance which complicates operations*? It is enough to hint these doubts; for they only touch upon the general question of anæsthesia, not upon the parallel between anæsthetic agents, to which the discussion confines us for the moment.

In these terms, in fact, was the dilemma stated at the Société de Chirurgie. "What do you ask?" it was said to M. Hervez de Chégoin, "To abandon anæsthesia? The thing is not possible, it is not proposable. To substitute another agent for chloroform? Let us see."

Let us see ourselves, and see closely. In the eyes of the justly eminent speaker, whose opinion carried with it that of the Société de Chirurgie, in the eyes of M. Gosselin, ether is not worthy of taking the place of chloroform. And why? I quote his reply literally and entirely:—

“Because it has the inconvenience of requiring more time, of causing a noisy, talkative intoxication, sometimes an erotic one in women, and of necessitating a special apparatus. Moreover, it does not procure a profound anæsthesia, muscular flaccidity, which we cannot dispense with at the present day for straightening limbs in ankylosis. If we attempt to produce an anæsthesia of this kind with ether, we expose the patient to the danger of death quite as much as with chloroform.”—(*Société de Chirurgie*, meeting of March 10th.)

We cannot doubt that these arguments were intended in good faith; but it will be more difficult to accept them as founded in reason. A great historical fact, and a great experimental fact, render it unnecessary for us to refute them at length. The first is, that all the principal indications of anæsthesia had been already discovered and fulfilled by means of ether; chloroform developed nothing new of importance, it merely enables us to act more expeditiously and conveniently. The second fact was demonstrated by the uniform statements made before our medical society; for a number of years ether has been exclusively employed at Lyons, in the hospitals as well as in private practice. But has surgery on this account been disarmed in the special operations which have been alluded to? We may ask the Société de Chirurgie itself whether the articular extensions which Prof. Bonner obtained by means of ether alone, come under the class of operations for whose success a profound anæsthesia is necessary? “Surgery,” said M. Robert, “would no longer be possible without chloroform.”—(*Moniteur des Hôpitaux*, April 2.) Surgery impossible! And what have we been doing, if you please, in Lyons, these eight years?

But let us discuss only the question of death. In Paris, ether is only remembered as a historical curiosity. It is said, naturally, this agent is quite as capable of killing as chloroform. It has several times caused death when, at the beginning of anæsthesia, no one went to sleep without it; and if, since 1850, hardly any cases of death are reported except from chloroform, it is not because ether is safe, but simply because it is no longer employed as an anæsthetic.

To this we have to make two replies: one rational, debateable; the other of fact, peremptory. In the first place, of the evils attributed to ether, the greater number, with three or four exceptions, may be differently explained. They evidently *follow*, but are very improbably the *consequence* of the employment of the anæsthetic. In most of the cases, the fatal termination was not instantaneous, as is always the case from chloroform. We might

almost call it a chronic or secondary death. Out of nine facts of this kind which I collected toward the end of 1848, the lapse of time between the moment of etherization and that of the cessation of life, was 3 hours, 5 hours, 8 hours, 25 hours, 34 hours, 3 days, 6 days and 15 days! Who would venture to assert that a given cause had alone produced death, when the patient survived almost always several hours, often several days, the action of this cause? The second answer should suffice for demonstration. Lyons has not imitated other surgical centres in making her choice between the two anæsthetics. While elsewhere men were hurried on to extravagance, we kept to the safest doctrine. After a few calamities from chloroform, witnessed by ourselves, we were sufficiently warned. For about eight years, ether alone has been employed at Lyons, either in private or in hospital practice, and during that time the necrology of anæsthesia has remained closed here, at least if I may judge from the universal declaration, and from the silence of those of my colleagues who hastened, in so praiseworthy a manner, to publish, at the beginning, their disasters.

These reflections, of which I willingly assume the responsibility, although they but express, perhaps in more distinct terms, the feeling which has been created in the midst of the Society of Medicine—these reflections, I say, developed a moral force which was not destined to remain long in a potential state. The Society determined that they should serve as the basis of a formal declaration of principles, possessing all the authority which the consecration of a vote could confer.

A more radical idea was started; it was proposed, in order to be still more explicit, and to effectually restrain those who were not sufficiently guided by their consciences, that the presumption of *imprudence* should be attached, by vote of the Society, to any one who should hereafter employ chloroform in a case where he might have used ether. The proposition found no echo, for which it must seek its own consolation. We would have sustained it, if it could have been thereby less isolated. The Society chose to confine itself to its purely scientific mission. It did not desire, at any price, that the opinion it was about to express should be invoked in any other sense than that of the interest of physicians and of patients. At the same time, and with the more confidence in its influence that it maintained it within just limits, it made no sacrifice to the counsels of an exaggerated prudence. A formal statement, without possible action (at least for the present), was the evident result of the union of so many consonant opinions: *Ether is capable of fulfilling the same indications as chloroform, without exposure to the same danger.* M. Barrier, the judicious instigator of the discussion, considered that there was reason for notifying, so to speak, to the scientific world, this statement, in order that it might exert upon all our brethren that influence which, without having been distinctly enunciated, it has already

had upon the convictions and the practice of the physicians of this Province. He consequently proposed, as a corollary to the discussion, the following conclusions:—

The Imperial Society of the City of Lyons is of the opinion:

That ether employed to produce anæsthesia in surgery is less dangerous than chloroform;

That anæsthesia is obtained as constantly and as completely by ether as by chloroform;

That if ether presents the inconveniences which chloroform offers to a less degree, these inconveniences are of slight importance, and do not compensate for the danger inherent to the employment of the latter;

That consequently, ether ought, in general, to be preferred to chloroform.

These conclusions were unanimously adopted.

"NEVER TOO LATE TO MEND."

[Concluded from p. 113.]

CASE III.—*Ovarian Disease.* Mrs. W., aged about 50. Married at 22; has had no children. Good health until 1846, when she had soreness and pain in the right iliac region, and upon examining that part felt a small tumor about the size of a nutmeg, which grew fast, and at length acquired great bulk. So large was it, that a Professor Somebody or Something, of much fame, was called in. He said it was a bag full of water, which filled her belly, and if it were not cut out it would kill her. But she declined the operation. She said further, that she was at that time much larger than when I was called in, and that the water, of the presence of which there was no doubt, had disappeared. From her account of it, I was inclined to think it was ascites; or if a cyst had existed, which might have been the case—a tumor existing at the same time—that the sac had been ruptured, and the fluid absorbed.

I first saw Mrs. W. in September, 1858. A large and very solid, hard tumor occupied the right iliac region and hypogastrium, from which to the left a large mass extended, there being a deep sulcus between them. She was by measurement 37 inches round in the largest circumference. There was not the least fluctuation anywhere, and the intestines were entirely free from flatus. She was incommoded by the weight of the tumor, but had sufficient flesh and strength. She was treated as were the preceding cases, with the liquor calcei muriatis and tincture of iodine.

This day, July 27, 1859, I measured her with the tape which was used last September. The date was then marked on it in pencil, with a line indicating the exact size of the abdomen, then, and at different times since. Her size is 25 inches, 12 less than a year ago. In the last two months she has lost 2 inches. Her general health is good—flesh sufficient—and she is able to work.

The mending in this case is not complete, but, considering all things, the repair is quite respectable. If nothing happens, as the phrase is, she may yet get well.

Two cases follow, which may not come exactly within the limits of my text. This sometimes happens in other preachings. If, however, you think "they will serve," you are quite at liberty to use them.

CASE IV.—*Ovarian Disease.* Mrs. —, aged about 40. Has had no children. Dropsical ovary—of slow growth—without any constitutional lesion. Appearance healthful—sufficient flesh—complexion clear—expression cheerful—bright. I have never met with a case so declared as was this, in which there was so striking absence of general disturbance.

The abdomen was filled by the ovarian distension. Fluctuation was perceptible, and very distinct everywhere. There was habitual costiveness, and occasional dysuria. The bulk and weight of the tumor embarrassed exercise. This seemed a case in which hydragogues might be tried, and they were given. The abdomen was carefully measured. Purging was well borne, and as size was diminished, and Mrs. — had no fears of medicine, she never took less than was directed. She had faith in physic, and she had sight of its salutary uses. She heard of a lady in Baltimore, who had been safely treated by an operation, whose case exactly resembled hers. She went to Baltimore, saw the lady, and the ovarian sac, it having been carefully preserved by her, for she felt that it belonged to her.

Mrs. — came home, and determined to have her sac removed, and Dr. Kimball, of Lowell, performed the operation. He used the short incision. A sac protruded as soon as the abdomen was opened. It was punctured, and as soon as it was emptied another sac appeared, and was emptied, and then a third. They were now raised by an assistant and held up, when a doubly armed needle was passed through the base, and the ligatures having been tightly drawn and tied, the empty sacs were severed at the base from whence they arose. Convalescence was rapid and without the least accident, and recovery was soon established. Etherization was employed. Not long after recovery, another tumor appeared in the opposite side. It grew rapidly, and Dr. Kimball being again called, and dropsy of the ovary being diagnosticated, he performed an operation similar to the first, the result of which was as rapid, and as perfect, as in the first.

There are many points of interest in this case. Among these, are the recoveries, and the absence of all disease from the operations. There were no adhesions of the sacs to the walls of the abdomen, or to any of its contents. Mrs. — was apparently in perfect health, as much so immediately after the operation as before. Cathartics were followed by diminution in size, which was established by careful admeasurements of the abdomen. The

highest authority considers it to be established that the lining tissue of an ovarian sac is not, and cannot be an *absorbing* one. Mrs. —'s case must be regarded as exceptional, for the sac was clearly diminished in size, during very active purging. If my memory serve, hydrocele has spontaneously disappeared, or has gone off during treatment—local treatment. Is there not some analogy between this disease and ovarian dropsy? While the patient is comfortable, the radical operation is deferred. In Mrs. —'s case the operations were done while she was in *perfect health*. This is a point deserving serious consideration. Was not this condition a cause—an important agent in the recovery? Mrs. — was of extraordinary will—of great firmness. She chose the matter of operation, and felt sure of recovery. The result abundantly confirmed her prognosis.

The second operation was done not long after the first, and during perfect health, and with a like good result. Do not these facts go far to show that the common delay of the operation is among the principal causes of its frequently untoward result? We go on tapping, tapping, until it is clear that death, if not at hand, is not far off. What more unfavorable circumstances can exist than the universal disturbance, and local sufferings, during which ovariectomy is commonly done? I know the reasoning for delay. I know well how fatal has the operation been with us, and that success in one case in three is thought large abroad. Would it not be larger if the operation were done earlier? And again, should it ever be done in the desperate conditions supposed? I have never seen ovariectomy successful except in the two cases above briefly sketched. Mrs. — had never been tapped, and she was in perfect health; her great size being an inconvenience only. I know a surgeon, for whose knowledge, and for whose opinion, I have the sincerest respect, who I have heard has said that he would never do ovariectomy.

Uterine Dislocation.—July 19th, 1859. I was called July 8th, to see Mrs. P., aged 35; married. First child born July 30, 1858. Did not nurse. Catamenia regular till February 28, 1859, when it ceased, and she supposed herself pregnant. About 7 weeks before I saw her, reports that something protruded from external labia; had been much fatigued by work on the day it appeared. Did not call assistance, as she recollected that in her first pregnancy something of the same kind occurred, but differed from this by receding on her lying down, whereas in this case it had remained constantly protruding and increasing in size. About a week before I was called, viz., six weeks from the first appearance of the tumor, Dr. — was called in. He attended her for a week, but Mrs. P. getting no relief from his remedies, and he feeling, as he told his patient, very uncertain as to the nature of the protruding mass, left, recommending Mrs. P. to call on me. The tumor was getting very un-

comfortable to her, making sitting very painful; and a purulent discharge from its surface was now complicating the case.

Upon examination, a large tumor, having two projections, in the sulcus between which was the os uteri. This last was stretched out by the swelling, so as to seem more than an inch in length, having wrinkles or radii going from its edges, or thick rounded lips. It is regretted that the protruding mass was not measured in its circumference, for, had it been, the state of the os uteri would be better understood. The tumor was in parts of it soft, and exactly feeling as if containing a fluid, which doubtless was the case. In other parts it was firm, and unyielding. Its color was blueish, or livid, as Parent Duchatelet says is the color of the vagina and neighboring parts in pregnancy. It may have been owing to the partial strangulation of the mass by the distended external organs. The examination gave no pain.

Mrs. P. believed herself between four and five months pregnant. The signs relied on were cessation of the menses, nausea, the protrusions at the external organs which accompanied her first pregnancy in the early months, and foetal motions. My examinations favored this belief. Firm abdominal fulness existed, and when the finger was carried upward along the surface of the tumor, and it could go no further, a firm resisting mass was distinctly felt, exactly resembling the gravid womb.

In deciding what was to be done, it seemed in the first place very clear that the protruding mass belonged in the pelvis, and the indication was quite as clear that the sooner it got there again the better. To answer the indication, the tumor was seized in its length by both hands, one not being sufficient to receive it, and was by gradual pressure so far diminished in size as to lead to the belief that it might be carried upward, or within the external organs. The direction given to the ascending mass was forward, the end of the tumor itself being carried toward the perineum. It soon began to return to its place, and was at length completely reduced. The finger now being carried along it, reached the womb, enlarged, and certainly very firm; and in its whole feel entirely unlike the formerly protruding mass.

Now what was this mass? The cervix uteri was greatly enlarged and lengthened. The fluid within it led to the thought that the bladder might be dislocated, and the catheter was used. But a very small quantity of urine came away, and the instrument as soon as it cleared the symphysis suddenly and strongly passed forward toward the anterior wall of the abdomen. The diagnosis got confirmation from the changes which the tumor underwent after reduction. Mrs. P. was visited the day following, when the mass, or cervix, was found to be sensibly diminished, and in subsequent visits was found to have nearly acquired its natural dimensions. A T bandage, with a thick compress to the external organs, was applied, and the patient directed to remain in bed, and to avoid all unnecessary exertion.

A question may arise as to the existence of pregnancy. The opinion of Mrs. P. on this point, is in the history. She has certainly furnished a new sign—*procidencia uteri*—of that occasionally embarrassing condition; but in one example only.

Incontinence of Urine.—Miss —, between 30 and 40, works in a shop, sitting all the time. Goes early, carrying her dinner, and does not leave work till evening. No water closet in the premises.

"They manage these matters better in France." He or she of our own city, who finds him or herself, after less than a fortnight's voyage, in Paris—that city of magnificent accommodations in all kinds—may take breath, and feel there more than at home.*

But to my story. Miss — was sorely tried by her first experiences in her place of work. The bladder and its earnest solicitings were wholly neglected. No matter what was the demand in amount or frequency, the evil must be endured for the present, and the future. At length habit made tolerable what was certainly bringing terrible harm and suffering. The time for which the urine was retained seemed to me incredible, but Miss — was a competent witness in every sense of the word. At length the bladder began to provide for itself. In other words, the urine began to take its own course—to act without the will. This was not in some respects agreeable, but the relief was compensation enough for the time. The balance, however, soon got disturbed, or was on the wrong side, and gradually the whole system began to tell its story. In few words, she had to give up work—to keep at home—and at length to go to bed, and to keep there. I was now called. The foregoing history was given, and in addition to involuntary urinations, it was found that the bladder was gravely diseased. There was blood, and a heavy mucopurulent addition to the urine; there was constant pain in the bladder, and surrounding external and internal tenderness, and sharp soreness. We rarely meet with a more pitiable object than was this poor woman. It was serious, too, to have to give up work, for by that she lived. Costiveness existed in its extreme degree. This had come of her irregular and sedentary life.

Under treatment the bladder recovered, so far, at least, as not to be painful, and the urine grew natural. Much difficulty was met with in overcoming costiveness. I have long used for this, a prescription of Mr. Henry Earle, a distinguished London surgeon,

* Let me give an illustration. I had, one beautiful morning in Paris, reached the Station for the South of France, on my way to Madrid. The Station is near to the Garden of Plants. My attention was attracted by a notice which was at once understood. A woman stood by a door with a key in her hand. I approached her. "Do you do both, sir?" said she. "No; one." "Here," said she, pointing to a door. A sous, and the dialogue was ended. Now this I call luxury. I am told that a Minister Plenipotentiary who had just got home, here in Boston, once availed himself of a corner, which our staid ones thought was a custom more honored in the breach than in the observance amongst us, who are native here and not to the manner born; and which a modern Dogberry might have looked into. One of our most honored physicians has done much good by getting accommodations in establishments in which women are employed; and you, Messrs. Editors, have earnestly called attention to more public arrangements for the same most important object.

many, many years ago. R. magnes. sulphat., \mathfrak{z} i.; magnes. calcinat., \mathfrak{z} ij. M. ft. chart. no. iv. Of these, one is to be taken before breakfast, in a teacup of gruel. I have used this cathartic for many years in my walks, and have for the most part found it excellent. At times larger doses may be necessary. Two or three times a week is often enough, with longer intervals and smaller doses as the bowels get into better state. For some renal troubles, these powders do well.

Incontinence continued in Miss M.'s case, notwithstanding the improvement in other symptoms, or states. I now consulted Dr. Morland's excellent work on the Urinary Organs. It had just appeared. I here learned that cathartics had been found useful, nay, curative, in urinary incontinence, and notwithstanding such purging as had followed my treatment, I was determined to try something more. It occurred to me that a favorite cathartic of the late Dr. Samuel Danforth—a physician amongst us of the highest fame in his day—his *Ten* and *Ten*, so called with his subsequents, might do something, and the following was written for: R. Hyd. sub. mur., pulv. jalap., aa gr. x.; muc. acaciæ gum., q. s. M. ft. pil. no. iv. The direction was to take these at once; and in four hours, if no purging, R. Ol. ricin., suc. limon., aa \mathfrak{z} ss. M. This was to be repeated in four hours; and if no free dejections, an enema, according to the Hospital rule—soft soap, olive oil, &c. &c.

The pills were taken, the oil and lemon juice prepared—but just before the last hour was accomplished, hints of intestinal action declared themselves, and too emphatic were they to be questioned. Seven dejections rapidly followed each other. These were of black indurated balls, sharply scraping as they came. Then a short repose, and lastly two most copious, soft-solid, and liquid stools.

Miss M. declared to me that there was never anything which equalled this purgation. She was a new creature. The bladder gradually came into correspondence with and obedience to the will. She could do as she pleased. She went into the country to recruit, and returned to her old work of "stitch—stitch—stitch;" got into good quarters, with steady and well-paid-for employment, and excellent necessary accommodations.

When William Hunter, having told his class, one day, that he once had a patient who from a most mismanaged labor had extensive mortification and sloughing produced of vagina, perineum, rectum, &c. &c.—and notwithstanding all this had recovered—he concluded by saying, "Gentlemen, do not call this one of Hunter's stories, but learn from it the importance of proper treatment of your cases of difficult labor, and have faith in medicine, under whatever circumstances."

Now, dear Editors, do not call the above one of my "stories," but do all you can to remove the occasion of so much danger and distress as Miss M. encountered; and, above all, notwithstanding Sir

John Forbes and Mr. Bennett, have faith in physis, even to the use in your practice of TEN and TEN.

In my Note Book—"the book and volume of my brain"—are two cases of polypus uteri, which, from the manner of their termination, may be worth a word or two:

CASE I.—Mrs. —, married—had long been flowing. She grew sallow—yellowish under loss, and her disease was called "Liver," and treated accordingly. She grew very weak, which was ascribed to "loss of strength"—not to the other *loss*. For this, exercise—out-door-exercise—driving, a generous diet, tonics, &c., were directed. But liver treatment, and exercise, did no good, and so in due time she kept house, and then bed. Another physician was called in. He made an examination—this not having been done before—and found polypus. He requested me to see Mrs. —, and a ligature was passed round the tumor. The instrument was returned to me, and attached to its loop-end was the polypus. The ligature had been drawn until it ceased to render, and the part of the tumor which it encircled was of no estimable diameter. Absorption had taken place toward an inch *above the ligature*, and there was the spot of separation. She recovered perfectly.

Mrs. —, aged 47, has had but one child, now 14 years old. For eight years has flowed most at periods, but often in intervals, especially under exertion. At length, having broken down, she desired that I should be sent for. The messenger—a layman—described the case so clearly, that I was well satisfied as to its nature and carried my instrument for polypus with me. Upon examination, anteversion and a polypus were discovered. A ligature was applied. As the end of the polypus looked strongly to the hollow of the sacrum, some difficulty was experienced in passing the ligature round the tumor, of which an inch or two only had passed the os. The family physician drew the cord twice daily, and when I went out on the third day, it was clear that decomposition had satisfactorily occurred. Several inches had been added to the ligature by drawing. I went again on the seventh day, and drew the cord myself. It did not render, a line. The instrument was now put upon the stretch, and was found very loose, coming down easily at first, then stopping, and a dragging sensation was felt by Mrs. — at the lower part of the abdomen. A finger was now passed along the canulæ till their ends were felt. They were in close contact, and nothing like a loop could be felt. Leaving this point, a portion of the tumor was reached, which was small, round, and of some consistence; and still further on, the remaining and uterine portion was felt. It was now clear that the separation, as far as accomplished, of that portion which was below the ligature, out of the womb, had not taken place by the action of the ligature, but above it, and by absorption—the living part having nearly thrown off the dead one. Some effort was now made by the

finger to complete the separation, which was soon effected. At the moment this was done, Mrs. — made a slight, sudden start, as if fully sensible of what had been done. The canulæ were now drawn out, and hanging at the end was the tumor, the separation having occurred half an inch or more above the ligature, and exactly resembling what was observed in the first case. This happened a week and a day from yesterday, Sept. 2, 1859, and about a week from the application of the ligature. I found Mrs. — in a perfectly good state of convalescence, and directed a strict perseverance in the horizontal position, as I have known most alarming syncope, and in one case death, to have occurred from premature sitting up after operations in uterine disease accompanied by long-continued and large hæmorrhages.

The physiological interest of these cases is the mode in which the tumors were separated. The practical one is, the importance of ascertaining why the instrument does not come away at once upon being drawn, if it be apparently detached—the ligature having ceased to lengthen when drawn, and the canulæ coming down an inch or more with perfect ease, but still showing a connection with that to which they have been applied, by receding more or less when the strain is taken off. Days in Mrs. —'s case would have passed, had not the cause of retention been discovered, and removed. I do not remember any recorded case similar to the above, and am sure that none like them have occurred in my own practice. I have heard of a polypus remaining in the pelvis eight days after the canulæ have come away; another, for two or three days. The physician, in the first, had not at the time the means of seizing the very large movable mass—the second being an intra-uterine polypus, and which could not be successfully reached. The last patient finally got out of bed upon a vessel, and by strong voluntary effort expelled the tumor.

We have been talking of fleshy tubercle in the polypus, and in ordinary simple uterine enlargements. It is polypus when occupying the uterine cavity, and simple tubercle when occupying the cavity of the peritoneum. In both, the disease is the same. I have met with one instance in which both polypus and common uterine enlargement existed in the same patient. For preventing hæmorrhage, the polypus was removed. Ascites further complicated this case, and when I last saw Mrs. — she was sinking under her many troubles. Enucleation now and then occurs, for the fibrous tumor is quite distinct from the peritoneum and uterine mucous tissue. In one case, enucleation had begun toward the cavity of the womb, as I discovered after death, this being caused by uncontrollable hæmorrhage. In the other, enucleation was accomplished after death, the tubercle being very easily detached from both peritoneum and mucous membrane.

Messrs. Editors, I have no doubt both you and your readers

will rejoice at having at last reached "Land." You recollect the old Greek story of an audience listening to a most long and tedious work. I will not tell it, but only add,

I am very truly yours.

Boston, September 5, 1859.

W. CHANNING.

Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT. BY F. E. OLIVER, M.D., SECRETARY.

MAY 23d.—*Hydatid Placenta.* Case reported and specimen shown by Dr. C. E. WARE.

The patient was a healthy woman who had been confined three times; the pregnancies having been in every way normal. At the end of the second month of the present term of pregnancy, she was threatened with miscarriage; there being pain (in the back) and hæmorrhage. She was examined by her physician, who supposed the placenta to cover the os. She, however, recovered from the attack, but at the end of the third month was again attacked with similar symptoms—at which time Dr. Ware saw her.

Under the use of opium and rest these also passed off, and she again got about.

At the end of the fourth month she thought she had quickening, the sensations being noticed, however, but two or three times afterward. At the end of the fifth month, she again had hæmorrhage, which was not severe, and which soon ceased, the patient being in other respects quite well.

At the end of the seventh month, there was an attack of severe pain, but little or no hæmorrhage.

Dr. Ware now examined her, and found a soft mass within the os uteri resembling the placenta. This he succeeded in separating; and in an hour and a half it came away. The abdomen did not increase in size after the fourth month. No embryo was discovered.

JULY 15th.—*Discharge from the bowels of the Appendix Cæci during convalescence from an acute attack.* Dr. JACKSON presented the specimen, and gave the following history of the case which he had received from the attending physician, Dr. JAMES ROBBINS, of Uxbridge.

The patient was a robust farmer, aged 24 years; habitually rather costive. On Saturday, the day before his attack, he had erred somewhat from his usual diet, and toward midnight he began to feel sick; nausea, with vomiting of all the food he had taken the day before, in an acid state, and much pain in the abdomen from distension. Early the next morning he was seen by Dr. R.; symptoms continued, and the abdomen was somewhat tender; had had two fecal dejections, not costive. Ordered small draughts of soda, aloetic pills with enemata, sinapisms and fomentations to the abdomen. During the next twenty-four hours he was attended personally by Dr. R., but got no relief. About 2 o'clock in the night he had a pretty severe chill, and, fearing peritonitis, Dr. R. took about a pint of blood from the arm, but the appearance was not particularly inflammatory, though there was some relief.

On Monday morning, he was about the same. Pain nearly constant, with paroxysms of increase. Whole abdomen considerably swollen, with no particular tumefaction; soreness most on right side; eructations of flatus in great quantities from the stomach; vomiting continued, and inability to sleep. Calomel was ordered, with opiates if required; and a blister to the abdomen. In the evening the blister had drawn imperfectly; had had a somewhat fecal defection, but there was little or no relief except that the opiate had procured some sleep. About the middle of the night, Dr. Ballou saw the patient in consultation; and fearing inflammation, he advised another venesection, but the blood showed no signs of inflammation. Dr. B. also advised pills of aloes, scammony and gamboge, a repetition of the fluid extract of senna which had previously been used, some doses of castor oil and some of oil of turpentine.

On Tuesday, about the same; vomiting perhaps rather less frequent; no defection. Enemata well retained, but returned unchanged. These last were repeated, variously composed, throughout nearly the whole course of the disease; and, repeatedly, Dr. R. introduced a flexible tube ten inches, and passed up about $\frac{3}{4}$ of fluid, by forcing it into the raised funnel-shaped extremity, but with no better effect. In the night he got but little sleep, and no essential relief; by the advice of Dr. B., the anodynes were discontinued.

On Wednesday morning, an infusion of tobacco (Div. to about a pint of water) was given as an enema, and the effects were strongly marked. In less than five minutes it was returned, with a large quantity of mucus; profuse perspiration came on; the pulse, which had ranged from 100 to 112, fell to 84, and there was a distressing sense of prostration; two defections followed, with considerable quantities of green mucus, but without feces or fecal odor. The symptoms, however, were rather improved. In the afternoon, Dr. Ballou again visited the patient, and a frequent use of the blue pill was commenced. A larger blister was applied at, or more probably before this time. The abdomen was still very full and tender, but no marked tumefaction was found in the right hypogastric region; eructations still annoying, though somewhat diminished.

On Thursday, Dr. R. discovered an oblong tumor, reaching from above the spine of the right ilium nearly to the groin. A blister was applied over its whole extent, as some parts of the surface had not been affected by the previous ones; and this was removed a day or two afterward. On Thursday night, defections of a fecal character began to take place, and for a day or two were quite numerous. The symptoms soon subsided; the tumor gradually diminished, and on the following Tuesday there only remained a small portion of it at the upper extremity.

Having discontinued his attendance upon the Tuesday referred to, Dr. R. was again called to his patient on the following Friday or Saturday. He was no worse, but his friends were alarmed at his having passed, during the previous night, a substance which Dr. R. found, on examination, to be the appendix cæci; "it was in a fetid, gangrenous condition, and there were in it some irregular openings." More than three years have now elapsed, and the patient's health has been perfectly good since the above attack.

Dr. Robbins remarked upon the free use of mercury internally and externally, and the absence of any evident mercurial action upon the

system ; upon the non-inflammatory appearance of the blood, though the intestine must have been inflamed ; and he questioned whether the appearance of the blood, as he observed it, was due to anything peculiar in the inflammation, or to a mercurial action, though this last was not shown in the usual way. In regard to an explanation of the case, he thought " that some foreign body, probably, became impacted in the entrance of the appendix, that it then became inflamed at that point, and the circulation cut off from the remainder ; that ulceration then supervened, and the whole body of it being dead, was sloughed off into the intestine." Physiologically, he thought the question might be asked—" of what use is the appendix ?"

Dr. JACKSON remarked that the case was unique, so far as he was aware. The length of the appendix varies in different individuals. In the present case it measures 3 1-8 inches, and there is a gangrenous opening about an inch from the free extremity ; the situation and the size of this opening being about what is usually seen in the disease referred to by Dr. Robbins. The line of separation is not transverse to the length of the appendix, but quite oblique ; and the edges have not the sloughy appearance that is seen about the perforation. Dr. J. said that, in the cases referred to by Dr. R., he had never found the foreign body at the entrance to the appendix, but generally midway ; and he could not but think that in this case its situation had corresponded to the perforation ; the cœcal extremity of the appendix he had generally found sufficiently healthy, and he regarded it as a very remarkable occurrence that a separation should take place at this part. That the disease was in some way connected with the presence of a foreign body in the appendix, he thought there could not be much doubt ; the local abscess that generally forms about the appendix in these cases, assisting in its separation and expulsion into the cœcum. The specimen has been presented by Dr. R. for the Society's Cabinet.

Dr. HODGES said that he had examined the specimen, and had no question that it was the appendix cœci. At a subsequent meeting, Dr. H. gave an abstract of a very remarkable case of intus-susception or inversion upon itself of the appendix cœci, and showed copies that he had made of two drawings accompanying a full report of the case in the *Edinburgh Medical and Surgical Journal* for March, 1859.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, SEPTEMBER 15, 1859.

EMPLOYMENT OF ETHER INSTEAD OF CHLOROFORM IN FRANCE.—The merits of ether in preference to chloroform as an anæsthetic agent are slowly gaining ground in the estimation of the medical public, and the time will surely come when the latter drug will be almost wholly abandoned. It is unsafe, in the most careful and most experienced hands. Again and again have efforts been made to explain the mysterious deaths occurring during its inhalation. By some, the impurity of the article is held responsible ; others speak of spasm of the lung,

congestion of the lung, syncope, obstruction of the glottis by the tongue, fatty degeneration of the heart, and other hypotheses. But however difficult it be to account for the death, the fact itself is plain enough, and the number of cases has now become alarmingly great. They can be counted by hundreds. Now, so far as we are aware, *not a single death has ever occurred in consequence of the inhalation of ether*; and when we consider how often this drug has been inhaled by incompetent and even ignorant persons, the fact is certainly very remarkable, though not more so than the obstinate persistence of surgeons in employing so dangerous an agent, when an equally efficacious and far more safe one is at hand.

We publish, to-day, a translation of an article from the *Gazette Médicale*, of Lyons, in France, upon this subject, called forth by a recent discussion of the comparative merits of the two substances as anæsthetics, in the Imperial Medical Society of that city. We doubt not it will be read with interest, and we trust it will be read with profit by many who are in the habit of using chloroform. The flimsy replies to the able argument of M. Hervey de Chégoin, in the *Société de Chirurgie*, will strike the reader with astonishment. Is it possible that mere convenience of administration can for a moment weigh against the danger of instant death? for, after all, this is the only real advantage which chloroform possesses over ether. The reply of the editor of the Lyons *Gazette Médicale* is conclusive to those who maintain that the most perfect anæsthesia cannot be produced from ether; ether has been exclusively employed, with perfect success, in every case in which complete insensibility and muscular relaxation was required, in that city, for the past eight years. We may add, that the same is true of Boston for a longer period. Except for a short time, ether is the only anæsthetic which has been used in Boston, ever since the first memorable demonstration of its effects in the Massachusetts General Hospital.

We wish to say a few words about the alleged effect of the inhalation of ether in giving rise to erotic ideas, especially in females. We believe that this effect, always rare, occurs no more frequently with this agent than with chloroform. We have administered it to a great many women, as well as men, but we have never seen any evidence which would lead us to believe that such results are produced by it. We know that these effects have been occasionally observed by others, but we believe that they are quite as often caused by chloroform. The case of Dr. Beale, of Philadelphia, who was tried, and most unjustly convicted of the rape of a young woman who was under the influence of chloroform, undoubtedly furnishes an example of this. It will be recollected that the patient was menstruating at the time, and was shortly to have been married. She testified that the dentist committed a rape upon her, while she was under the influence of chloroform, in the operating chair. No evidence, other than the statement of the plaintiff, was introduced to prove that any rape had been committed, and the circumstances were such that a nominal sentence was pronounced, and the prisoner shortly afterward was pardoned.

The only objection which can be urged against ether as an anæsthetic, is its inflammability. In cases of surgical operations about the mouth, in which the actual cautery may be required, it might become dangerous, and under such circumstances it might be justifiable to employ chloroform. The same remark will apply to operations about

the head when it becomes necessary to operate by candle-light—as in croup, for instance. With these exceptions, ether answers every purpose which can be fulfilled by chloroform; and we reiterate our firm conviction that its safety will ultimately give it the preference over the latter agent, in every part of the world.

PHYSICIANS' CERTIFICATES TO THE CAUSES OF DEATH.—The Board of Aldermen, on the 5th inst., passed an order directing the Committee on Ordinances to consider the expediency of amending the ordinance concerning the Public Health, so that the certificates of attending physicians shall be required before the interment of the dead. We are glad to see that the City Government is at last awakening to the necessity of a proper recognition of the causes of death. We have repeatedly urged the extreme importance of some measure which should put an end to the vague reports which are made to the City Registrar by undertakers, who in the majority of cases are obliged to apply to the friends of the deceased for the name of the disease which was the cause of death, which information, of course, in many cases, is erroneous. The following instance is but one out of several which have come to our knowledge. A patient died rather suddenly, after an illness of a few weeks of a vague and indefinite character. The diagnosis was difficult, and, as it proved, erroneous. It was thought, in the absence of any well-marked symptoms, to be a case of mild typhoid, or "slow" fever. The *post-mortem* examination revealed an aneurism of the abdominal aorta, which by pressing upon the spine had caused pain in the back, the chief source of complaint of the patient. The case was reported to the Registrar as one of "unknown" disease; but had the undertaker been required to obtain a certificate from the attending physician, the true disease would have been recorded, and a vitiation of the statistical returns would have been avoided.

There can be no doubt that a large number of errors of this kind are constantly committed, and the effect is really and positively injurious to the community. To those who have not considered the matter, it may seem of very little consequence that the causes of death should be correctly reported, or that they should even be reported at all; but it is now universally acknowledged by all who are conversant with sanitary matters, that it is of very great consequence to the health and well-being of the community that we should ascertain, as accurately as possible, the causes of death among us, in order that these causes may, so far as possible, be anticipated and prevented. Our City Government admits the importance of registration, but by a singular inconsistency does not provide for the accuracy of the returns—as if the value of the latter were not in exact proportion to their reliability.

We hope the Committee on Ordinances will report in favor of requiring a certificate from a physician before each interment. The undertaker should procure the certificate from the attending physician; or where there has been no physician in attendance, one should be called in, to certify to the cause of death. This plan is pursued in the city of Providence, and with the best results. Dr. Snow, the Registrar, says, in his Report of Births, Marriages and Deaths for 1858, that "the returns of deaths during the past year were full and accurate. Indeed, we have no reason to expect any great improve-

ment in this respect, in future. During the year 1858, the date of death, name, age, place of death (including street, number and ward), the sex, color, condition (whether married, single, widow or widower), the birthplace and parentage, were obtained and recorded, of every individual who died in the city. There were no blanks or 'unknown' in this information. The same information has been obtained, with equal fulness, during the past three years. The physicians' certificate of the causes of death are obtained in nearly all cases where physicians have seen the patient, and thus we have the best evidence that can be obtained in relation to this important portion of the statistics." We have often had occasion to praise the accuracy and completeness of the vital and mortuary statistics of Rhode Island, and particularly of the city of Providence. There is no reason why our own should not be equally complete, and, as based upon a larger number of facts, more valuable. In order to make them so, it is of the first importance that the causes of death should be correctly registered, and this can only be done by requiring a certificate from a physician in every case, before giving a permit for interment.

THE CASE OF DEATH FOLLOWING THE BITE OF A TURTLE. *Messrs. Editors*.—Seeing, in the last number of the JOURNAL, that you have noticed the death of a boy in Plympton from the bite of a snapping turtle, I send you some particulars of the case, that you and your readers may judge whether the death was from or merely after the accident.

On Sunday morning, July 9th, I was requested to see the lad to which the *Gazette*, from which you quote, alludes. He was a bright little fellow, about 11 years old. He sickened on the morning of the preceding day, with the usual symptoms of fever—chills, headache, lassitude, &c.

When I saw him, forty-eight hours from the attack, he complained of some pain in the head, was unusually restless, with the tongue covered with a brownish coat, and with the pulse from 150 to 160 per minute, the restlessness presenting something of a paroxysmal character. The right thigh was somewhat painful, slightly tender to the touch, and a little enlarged. I saw him again on Monday morning, and found the swelling, tenderness and pain of the thigh nearly gone, but the other symptoms had all increased in severity. On my arrival on Tuesday morning, the pulse was not perceptible in one wrist, and, though felt, could not be counted in the other. The paroxysms of restlessness had wonderfully increased in violence, the boy tearing with his teeth the sleeve of his shirt, the pillow case and whatever he could get hold of, but never attempting to bite the attendants. There was no difficulty of deglutition, and he swallowed water with avidity. Although, when spoken to, he would give intelligible and coherent answers, he would immediately relapse into a kind of half cry, alternating with screeching, and attempts to tear the clothing with his teeth. He complained of but little pain. The ring finger of the right hand was swollen, and looked like going on to supuration. I made an appointment to see him in the evening, but other engagements prevented, and on Wednesday morning found him dead. He died at about four o'clock in the morning, the sixth day of the disease.

Some five or six days previous to his sickness, while playing with a small spotted turtle which he had found in a little pond near the house, the fore-finger of the left hand was bitten. He had been prying open the turtle's mouth, to feed it with pea-nuts, and when bitten, was teasing it by poking his finger at it. The finger swelled considerably, but there was no considerable pain. So little trouble had it given him, that the family had forgotten the occurrence until the morning of the sickness, when the man with whom he lived asked him how his finger was, and he replied that it was well enough, but, upon examination, about half a teaspoonful of matter was found to have collected.

The patient residing some six or seven miles from me, no *post-mortem* examination was made. I have heard incidentally—with how much truth I know not—that the head and whole body were badly swollen before burial.

I have never known or heard, before, of any serious results from the bite of a turtle. Would the fact of its having been teased and irritated for a long time increase the poisonous effects of its bite? I think in a dog it will do so. C.

Middleboro', September 5, 1859.

Deaths in Boston for the week ending Saturday noon, Sept. 10th, 98. Males, 52—Females, 46.—Accident, 5—anaemia, 1—apoplexy, 1—inflammation of the brain, 1—cancer in stomach, 1—cholera infantum, 22—consumption, 14—convulsions, 1—croup, 1—dysentery, 5—diarrhoea, 1—dropsy, 4—dropsy in the head, 4—drowned, 1—debility, 1—infantile diseases, 8—puerperal diseases, 2—scarlet fever, 2—typhoid fever, 1—disease of the heart, 2—inflammation of the lungs, 3—marasmus, 3—measles, 1—old age, 2—palsy, 1—premature birth, 1—smallpox, 3—suicide, 1—teething, 3—tumor, 1—whooping cough, 1. Under 5 years, 54—between 5 and 20 years, 5—between 20 and 40 years, 19—between 40 and 60 years, 12—above 60 years, 8. Born in the United States, 71—Ireland, 21—other places, 6.